

# The Global Methane Budget: 2000-2017

Sauniois et al., ESSDD, 2019

## Detailed Response to Anonymous Referee #1

We acknowledge the referee for the time spent on reading and commenting on the paper. We thank him for his useful corrections and suggestions on the paper, which have helped clarifying and improving the manuscript. Below are the responses to his comments (in italics, blue). Changes in the text follow each response in bold font.

In particular, the text has been shortened by 5 pages (8%), from 61 to 56 pages in the ESSDD format.

### General comments

*Thank authors for massive compilation effort. Important product! One hopes a large community will see and use. Good product for ESSD.*

We thank the reviewer for the compliment.

*Push data out to 2008-2017 decade? Various valid reasons for this but scattered somewhat randomly through the narrative. Pull all of this information into a short clear statement very near the top of the manuscript to explain time span and time lag?*

We included a subsection in the methodology – Sect.2.2 “Period of the budget and availability of data” to justify the analysis period to 2017. In this Section, we explain:

- Why some inventories were extrapolated to push the budget to 2017
- That surface models were runs over 2000-2017, covering the full period of the budget using dynamical wetland extent
- That inversions were also run until beginning 2018 to cover 2000-2017

*I and many others will want to use this in classroom. We might assign paper as background reading. Students will loudly object - too long! This reviewer agrees. Too long by a factor of two, at least. Almost long enough to need an executive summary? See suggestion below.*

Indeed, the manuscript is a long one. We reduced its length following the reviewer suggestions, and removed excessive details and repetitions. **A table of content** has also been added prior the introduction to help the reader to easily find his section of interest of the layout of this - long – paper.

*In present organization, permafrost emissions sit within natural emissions. Technically, and increasingly so, permafrost emissions forced by permafrost thaw should instead account in the anthropogenic category? But if you move terrestrial permafrost sources then you also should move sub-marine permafrost emissions, boreal plant sinks or sources, etc. No easy solution but the current location will prove increasingly problematic.*

Permafrost emissions sit within natural emissions. This is also the case for reservoirs, managed wetlands, or wetland changes that could be attributed to human activities. Attributing emissions to either “natural” or “anthropogenic category is challenging. Also, some – many – “natural” emissions of methane are/will be perturbed by anthropogenic climate change. In our study, we define *anthropogenic* as emissions caused by direct human activities since pre-industrial/pre-agricultural time and *natural* as all other sources, some of them being perturbed by climate change.

We agree (and the second reviewer as well) that some clear definition need to be given to what is called here “natural “and “anthropogenic “. We addressed this by splitting the former Section 2.3

(Definition of regions and source category), in two sections and creating a Section 2.4 “Definition of source categories”. In particular we moved a paragraph from the beginning of Section 3 to Section 2.3 to better define the frontier between “anthropogenic” and “natural” sources.

After a short description of the different processes inducing methane emissions, the text shows the following definition of natural versus anthropogenic:

**“In the following, we present the different methane sources classified from anthropogenic or natural origin. “Natural sources” refer to pre-agricultural emissions even if they are perturbed by anthropogenic climate change, and “anthropogenic sources” are caused by direct human activities since pre-industrial/pre-agricultural time (3000-2000 BP, Nakasawa et al. (1993)) including agriculture, waste management and fossil fuel related activities. Natural emissions are split between “wetland” and “other natural” emissions (e.g., non-wetland inland waters, wild animals, termites, land geological sources, oceanic geological and biogenic sources, and terrestrial permafrost). Anthropogenic emissions contain: “agriculture and waste emissions”, “fossil fuel emissions”, “biomass and biofuel burning emissions”, assuming that all types of fires cause anthropogenic sources, although they are partly of natural origin (Fig. 6, see also Table 3 and 6).**

**Our definition of natural/anthropogenic sources does not correspond exactly to the definition used by UNFCCC following the IPCC guidelines (IPCC, 2006), where, for pragmatic reasons, all emissions from managed land are reported as anthropogenic, which is not the case here. For instance, we consider all wetlands as natural emissions, despite some wetlands being managed and their emissions being partly reported in UNFCCC national communications. The human induced perturbation of climate, atmospheric CO<sub>2</sub>, and nitrogen and sulfur deposition may cause changes in the sources we classified as natural. Following our definition, emissions from wetlands, inland water or thawing permafrost will be accountable in “natural” emissions, even though, we acknowledge that climate change – a human perturbation – may cause increasing emissions from these sources. Methane emissions from reservoirs are considered as natural even though reservoirs are human man-made, and since the 2019 refinement to the IPCC guidelines (IPCC, 2006; IPCC, 2019) emissions from reservoirs and other flooded lands are considered anthropogenic by UNFCCC.**

“

*Whether or not the authors intend, this reviewer concludes that we face a larger and more urgent geospatial rather chemical challenge. In short terms, we need to fix the wetland inland freshwater distribution and extent uncertainties as our highest priority, with understanding and quantification of atmospheric OH distributions, reactions and impacts as probably a second priority. If authors intend these priorities, they should state them explicitly? They can always revise in a subsequent version. If this reviewer developed a wrong impression, then authors need to reconsider information they presented and how.*

The authors agree with the priorities. However, addressing them involves different communities. Accurate split between wetland and freshwater extent would allow a better estimate of the bottom up budget; quantifying OH burden and distributions would impact the top-down estimate of the methane budget and the growth rate projected from bottom up estimates. Working on both issues should help reconciling bottom-up and top-down estimates. The last section – Section 6, has been revised to better highlights priorities and progresses needed on the methane budget.

*Topical editor suggested that I do a full review. Finding the track-changes version somewhere between daunting and impenetrable, I agreed. After plowing through so much material, however, I almost wish I had tried instead to follow the track changes version. Fundamentally, this is way too much material. Removal of the regional details to separate papers helps a lot, but the present version still represents way too much material. You want strong interest and to serve many users, but at present the too-long,*

*too-disorganized, too-detailed text represents a severe barrier to almost every reader and potential user. Amazing compilation effort, but you need to present it after and on the basis of an equally amazing editorial effort. The latter effort remains sadly missing.*

The authors thank the reviewer for his honesty. We agree that many repetitions occurred throughout the text and some parts suffer from too many details. We have followed the reviewer's suggestions to improve the organization of the manuscript and shorten it by 8%.

*Specific comments - long list and I know I missed some issues*

*Line 304: "the relatively small and variable number of studies" Do the authors mean a small number of studies with variable results? A variable number of studies covering small regions or sectors? Confusing.*

This has been rephrased as follow: **"considering that the number of studies is relatively small for many individual source or sink estimates »**

*Line 317: "processed in the same way" Does this phrase mean that monthly and annual emission data, from any source, were subjected to the identical spatial 1x1 gridding? But some source data exists at higher spatial resolution? E.g. would need some form of disaggregation? Later (lines 320, 321) a reader finds that calculations of monthly and annual means came from (came after?) gridding. Confusion between spatial and temporal averaging here?*

The files were provided at monthly or yearly scale. First a spatial re-gridding was applied to have all fluxes on 1°x1° grid at the original temporal scale. Then, annual and decadal means were computed. It has been rephrased as follows:

**"Gridded emissions from atmospheric inversions, land-surface models for wetland or biomass burning were provided at the monthly scale. Emissions from anthropogenic inventories are usually available as yearly estimates. These monthly or yearly fluxes were provided on a 1°x1° grid or re-gridded to 1°x1°, then converted into units of Tg CH<sub>4</sub> per grid cell. Inversions with a resolution coarser than 1° were downscaled to 1° by each modeling group. Land fluxes in coastal pixels were reallocated to the neighbouring land pixel according to our 1° land-sea mask, and vice-versa for ocean fluxes. Annual and decadal means used for this study were computed from the monthly or yearly gridded 1°x1° maps."**

*Line 332: "the regional budget presented in (Stavert, 2019)" should read as 'the regional budget presented by Stavert (2019)'? Proofreaders will not know what to do with these punctuation errors so authors must take care to express them correctly.*

This has been corrected. A thorough reading has been performed to check the punctuation and hyperlinks related to each citation.

*Line 336: In the version I read ("the TransCom inter-comparison map (Gurney et al., 2004)", the Gurney et al reference shows as a non-functional hot link, different to all other references?*

This has been corrected. A thorough verifications has been performed to check the punctuation and hyperlink related to each citation. Hyperlinks were all removed.

*Lines 358, 35: again some references as apparently hot links, others in standard (expected) format. Why?*

This is due to the use of a former list of citation from the previous version completed manually with the new references instead of using the citation software. We have corrected this issue.

*Line 359: "most of the inverse system the" Instead: inverse systems?*

This has been corrected.

*Line 373: “Methane sources and sinks” Because the (long and detailed) section 3 deals entirely with bottom-up (e.g. line 374) emissions, and because section 4 carries the explicit title of top-down (e.g. at line 1677: “Atmospheric observations and top-down inversions”), shouldn’t the title of section 3 read as ‘Bottom-up sources and sinks’? (By this point one hopes that most readers/users know that the discussion refers to CH<sub>4</sub>.) If section 3 does NOT deal solely with bottom-up sources and processes (like most readers, this reviewer loses track of bottom-up vs top-down deep in section 3, e.g. in the section on CH<sub>4</sub> lifetimes) then the reference to bottom-up at line at 374, 375 needs to change?*

Indeed Section 3 present the sources and sinks from the process perspectives and provide estimate from Bottom-up approaches. The title of Section has been changed to **“Methane sources and sinks: bottom-up estimates”**. We keep methane in the title of section 3 but removed it in the sub-section. See the final Table of Content.

*Line 395 to 400: good section on definitions used here vs those used by IPCC. This reviewer agrees with these choices but, more important, thanks authors for explaining the differences.*

We thank the reviewer for the comment. The definition of the categories and how they are named here (“natural” versus “anthropogenic”) are critical and challenging in our study. For clarity, we moved the whole paragraph (previously L 380 – L 400) to subsection 2.4 “Definition of source categories”. Some explanations were added to explain that other emissions such as “permafrost” are classified in “natural”, see response above.

*Line 436: FAOSTAT is annual or every 5 years? Annual, evidently, from Table 1?*

Yes. FAOSTAT provide annual values. This has been added in the text

*Line 457: “Although, country emissions” Delete the comma?*

This has been corrected.

*Line 472: Here the reader finds FAO-CH<sub>4</sub> at annual resolution so FAOSTAT (question above) must also provide annual data? Different to FAO land use inventories with 5-year update cycles?*

Yes FAOSTAT, named here FAO-CH<sub>4</sub>, is at annual resolution.

*Lines 475 to 503: Some deception here? None of the five sources provides actual 2017 data? Instead, authors have, by necessity, applied source-specific extrapolations to each of the five sources to reach 2017. So in fact we really only have valid bottom-up data to 2015 (perhaps, with a stretch, to 2016 for some sources) but for reasons we don’t learn, authors advertise (e.g. through the title) coverage through 2017. I applaud the desire to update from Saunier 2016 but the definition of “the most recent available year” (e.g. on line 302) refers only to atmospheric concentrations but falls short on all bottom-up source data? Authors should make explicit mention of this discontinuity? Authors, knowing the UN systems, live with these inherent reporting delays but readers/ users may not understand? Earlier and later the authors refer to differences between top-down total CH<sub>4</sub> emissions numbers and bottom-up numbers. This extrapolation represents an additional uncertainty factor? This discontinuity, e.g. 2017 data in some cases but extrapolations from 2015 or earlier in many other cases, impacts Figure 3?*

Indeed, the delay in updating inventories may not be well-known by the reader. We acknowledge that extrapolation of inventories is mentioned a bit late in text. We have added few sentences in the Methodology section to make this clearer. However, the extrapolations based on FAO and BP statistics are the best that could be done, waiting for updated inventories. EDGAR uses FAO data as activity data for agriculture, and data from IEA for energy related emissions. The EDGAR version v5.0 has been recently released but stops in 2015 for CH<sub>4</sub>.

Besides inventories, top down and biogeochemical models have been run until 2017, and the most recent literature is used for other bottom-up sources and sinks. We have added the following text in the methodology, Sect.2.2 “Period of the budget and availability of data”: **“The surface land models were run over the full period 2000-2017 using dynamical wetland areas (Sect. 3.2.1).**

**For the top-down estimates, we use atmospheric inversions with atmospheric measurements covering 2000-2017....”**

*Also, Table 2, biogeochemical models for wetland CH<sub>4</sub> emission all “were performed for the whole period 2000-2017” (Table 2) but forced with invariant wetland surface area maps? We know that wetland surface areas have changes substantially but do the data show changes specific to 2008-2017?*

Biogeochemical models have been run over 2000-2017 using dynamic wetland surface area, that include satellite-derived wetland areas, as stated in Section 3.2.1.” **The WAD2M dataset provides monthly global wetland areas over 2000-2017.”**

*Line 553: “agreement Despite the offset of the SSP scenarios compared to the recent inventories” Need a period between agreement and Despite. What does ‘offset’ mean in this context? Divergence? Divergence from SSP2.6 or 3.4 but tracking 8.5? Not clear what authors intend with this sentence.*

Indeed, this was not clear, we intended to say that discrepancies in absolute estimates between inventories and SSPs scenarios in the year 2005 should be left aside in order to focus on differences of trends. This has been rephrased to: **“...agreement. In the future, it will be important to monitor trends from year 2015 estimated in inventories and compare them to SSP scenarios’.”**

*Line 560: “transport” here but “transportation” already mention above (line 557). In one case you mean the act or process of transportation and in the other case the actual transport of fossil fuel products (e.g. via pipeline)? For readers who do not know IPCC categories, we need clarification here. The term “transport” has been changed to “road or non-road transport”, which should be more obvious.*

*Line 606, 607: “applying “Tier 1” approaches for coal mine emissions is not accurate enough” ?are not accurate enough?*

The subject here is “applying”, as gerund, so the verb here is “is”.

*Line 649: “and, could lead to”. Delete the comma?*

This has been corrected

*Lines 665 to 669: studies find emission underestimates due to inability to correctly account for CH<sub>4</sub> from fracking? Otherwise, why does reader find this particular sentence in this particular location?*

Indeed, this section was mishandled, and sentences on oil and gas general activities were attributed to the “shale gas” section. This section has been revised by:

- Deleting “conventional and “shale” splitting
- Reorganizing the text, to make clear that most of the discussion -including the underestimation in the inventories- concerns all type of gas production.
- Mentioning shale gas for two aspects: 1. The increase in shale gas production in the US 2. Potential differences in emissions factor between conventional and shale gas.

*Line 672: “abnormal operating conditions” abnormal = fracking? Abnormal = fracking done not according to regulations or best practices? Abnormal = some other type of fossil fuel extraction?*

While revising the text of this section (see response above), some sentences have been deleted to reduce the section. This one in particular.

*Lines 675 to 683: how does this section contribute to the overall or fossil -fuel-specific budget calculations? The sentence following (lines 684 to 685) provides a short sufficient summary which could replace much of the prior text?*

The text has been revised and shorten as explained above.

*Lines 724, 725: “the volatile solids component” This reader does not know the chemical meaning of the term ‘volatile solid’?*

A volatile solid is a substance that can easily transform from its solid phase to its vapor phase without going through a liquid phase. This term, whose definition is available in a dictionary, has been kept.

*Lines 723 to 736: highly redundant. Could reduce to the sentence on lines 727 to 729: “Ambient temperature, moisture, and manure storage or residency time affect the amount of CH<sub>4</sub> produced because they influence the growth of the microorganisms responsible for CH<sub>4</sub> formation”. For budget purposes, we don’t need more than that?*

We agree, this paragraph has been reduced. Three sentences have been deleted.

*Line 751: Start here while deleting the prior paragraph?*

Yes. Anaerobic conditions in rice have already been mentioned near the beginning of the text. Few sentences have been deleted here.

*Line 775: “the work of (Carlson et al., 2016)” you mean ‘the work of Carlson et al. (2016)’. Please take care with punctuation, as proofreaders will not know what you want.*

Yes. This has been corrected.

*Line 779: “northward shift of rice cultivation”. Unless cultivation practices change with latitude, a latitudinal shift will not necessarily change or reduce emissions?*

Indeed, the paper suggest that such a decrease could be partly due to shift of rice cultivated area from southern China (paddies) to northern China (dry rice).

*Lines 837 to 905: way too much information here! Please only include what we need to know to understand this/these budget estimates. Also we seem to have lost the useful end-of-section summary of what this budget effort concludes for this source?*

This section has been greatly reduced and unnecessary details and descriptions, removed. The authors do not include these changes here for the sake of length of the response.

*Line 979: by this point the reader has encountered anaerobic conditions many times (waste water ponds, rice paddies, bovine guts, etc.). Do we really need a new definition and explanation of anaerobic metabolism at every point. Reduce, s.v.p.*

The first sentence has been deleted. The processes related to methane transport in wetlands and non-wetland freshwaters are now mentioned once, at the beginning of Section3.2.

*Line 980: “limit oxygen availability and creates suitable redox” create, not creates?*

This has been corrected, then deleted. See comments above.

*Line 987: are these processes diffusive or advective? This reader does not understand the phrase: “molecular diffusion limited advection”*

This has part has been deleted – after correction, see comments above.

*Line 992: “transportation and are further regulated by” What are ‘further regulated’? The land-surface. models? The CH<sub>4</sub> emissions? The model parameters? Very confusing, please re-write.*

This has been rephrased to:

**“... and transportation. The models are then forced with inputs accounting for changing environmental factors”**

*Line 995: "(Supplementary Material, Melton et al. (2013); Bohn et al., 2015)" Again, inconsistent and incorrect use of parentheses. Relying on bibliometrics software from various contributors evidently does not work. I have pointed out a few such errors, skipped over many, and anticipate many to come. A waste of this reviewer's time to point them all out. Assign one of your co-authors to search for, evaluate and correct every parenthesis. Authors must take this responsibility. Proofreaders will not find nor know how to correct all of them.*

This has been corrected here and elsewhere.

*Line 1008: "a monthly global wetland area dataset" Does wetland surface area vary only seasonally or does it also evolve with time over several years?*

Here, it is both seasonal variation and dynamic over the 2000-2017 period. This has been rephrased as follows:

**"WAD2M provides year to year varying monthly global wetland areas over 2000-2017."**

*Line 1056 to 1074: a hard-nosed editor could re-write this entire section in two sentences or perhaps even one sentence. How much of this is relevant to this current version of a CH4 budget?*

This part has been reduced substantially as requested.

*Line 1107: "south of 60°N) with. Tan and Zhuang" something wrong with punctuation here?*

This has been corrected.

*Lines 1165 to 1174: here we get expert opinion on what next and what needed for lakes and rivers. Good! But doesn't this section belong elsewhere/later?*

As suggested, these lines have been removed from the budget Section, and used later in the discussion/perspectives.

*Line 1234: why specify exact (and proprietary) GIS software here? Same scale up would have worked in any GIS environment?*

Indeed, the detail on the software is not relevant here. The same scale up would be achieved with any GIS software. This has been removed.

*Line 1341: "which allows very little methane; even from established". Punctuation error?*

The sentence has been rewritten to:

**"Aerobic oxidation is a very efficient sink process, which allows very little methane from reaching the atmosphere even from established and vigorous gas seep areas or below-water gas well blowouts."**

*Line 1376, 1377: Authors already defined marine clathrates earlier (line 392). Reader does not need redundant definitions?*

Indeed, two sentences defining again clathrates have been deleted.

*Line 1398: In earlier sections readers found summaries first, then itemization of individual components following. Here we get the opposite: components first followed by summary. Chose one or the other but do not confuse us with both?*

Indeed, for anthropogenic emissions we discussed emissions by broader to finer categories, while for natural emissions, we do the opposite. We discuss estimate for each fine category and then combine... This way to proceed is more convenient for natural emissions, as few studies assess the broad categories but rather look into some specific source or process. The authors feel that the reader can accommodate to this potential issue.

*Line 1455: Many references in this section appear as (non-functional) hot links. Many other references do not. Please fix this!*

Yes, we worked on correcting this.

*Line 1511: Finding the (strange) unit here of “molec” and having earlier seen reference to mole fractions (strictly, in an SI units sense moles per mole but more casually expressed as ppb), one wishes the authors had provided a conversion table as in the global carbon budget. They have done a very good job of keep flux units consistent as Tg CH<sub>4</sub> yr<sup>-1</sup>, but they could help readers by cross-referencing the various concentration units.*

Methane emissions are given in Tg CH<sub>4</sub> yr<sup>-1</sup>

Methane mixing ratios are usually expressed in ppb.

OH concentrations in the atmosphere are commonly given in molecules cm<sup>-3</sup> in the literature as done here. The authors have not seen any occurrence of OH concentration in mole per mole unit in the literature.

In the CO<sub>2</sub> global budget, they provide conversion factors for CO<sub>2</sub> to C, and one conversion factor from carbon flux to carbon concentrations in the atmosphere (assuming many hypotheses, not valid here neither for CH<sub>4</sub> or OH).

As a result, the authors will keep the initial unit used in the paper. **“molec” has been replaced by “molecules”.**

*Lines 1498 to 1558: do we need an extensive discussion of OH concentrations, reactions, hemispheric distributions and lifetimes to understand the magnitude and validity of the present CH<sub>4</sub> budget? This review thinks not. One could summarize this entire section in two or three lines?*

Well, the authors understand that the text was missing a direct relationship between OH and methane loss. A sentence has been added to address this. (**“Mass-weighted OH tropospheric concentrations do not directly represent methane loss, as the spatial and vertical distributions of OH affect this loss, through, in particular, the temperature dependency and the distribution of methane. However, estimating OH concentrations and, spatial and vertical distributions is a key step in estimating methane loss through OH.”**)

The authors also acknowledge that the discussion was too long and sometimes off-topic (not the right time period, top-down instead of bottom-up...). As a result, many parts of the text have been removed.

*Line 1526: Pinatubo eruption represented a climate variation? Hardly.*

Indeed, Pinatubo eruption is not a climatic variation, but it did impact the climate for the following years. This has been deleted to avoid misinterpretation.

*Line 1530: “enhances CH<sub>4</sub> consumption”? You mean ‘enhances the CH<sub>4</sub> sink’ or ‘enhances CH<sub>4</sub> oxidation’?*

Yes. This has been rephrased to **“CH<sub>4</sub> oxidation”**.

*Line 1548: “consistent, albeit lower, than the value deduced from”? Very sloppy wording, leaves a reader guessing at what you mean here.*

Yes, this has been simplified to **“lower than the value deduced from”**.

*Lines 1577 to 1579: This sentence summarizes the entire stratospheric dynamics and photochemistry section. Why do we need all the rest of this?*

The reviewer is right. This section has been substantially reduced to keep only what is relevant to the stratospheric methane loss estimate.

*Line 1590: Should CH<sub>4</sub> sink terms use a different sign than source terms? I suspect carbon budget has already wrestled with this flux direction issue. What did they decide? Does GCP adhere to a consistent sink/source nomenclature convention?*

Here both emissions and sinks are provided as absolute values (positive), as done in the CO<sub>2</sub> global budget, where all values given in the tables are positive.

*Line 1606: “the KIE approach” KIE = kinetic isotope effect, but you haven’t defined it yet?*

Indeed, KIE is now defined near the beginning of the section, when it is first mentioned.

*Line 1679: ~~The first systematic atmospheric?~~*

This has been corrected.

*Line 1695: A couple TCCON sites, perhaps Australia or NZ or both, have published CH<sub>4</sub> data in ESSD?*

Yes? There is a Pollard et al. (2017) paper that has been published in ESSD. This citation has been added. Pollard, D. F., Sherlock, V., Robinson, J., Deutscher, N. M., Connor, B., and Shiona, H.: The Total Carbon Column Observing Network site description for Lauder, New Zealand, Earth Syst. Sci. Data, 9, 977–992, <https://doi.org/10.5194/essd-9-977-2017>, 2017.

*Lines 1699 to 1711, other data. If not useful or used for this global CH<sub>4</sub> budget, why mention it/them?*

Indeed, the previous manuscript has been already amended in this direction but not enough. The text on “other” data has been removed.

*Lines 1713 to 1716: a reader just saw this identical list a few lines earlier. We don’t need to see two mentions of the same information?*

Indeed, this has been reduced to:

**" We use globally averaged CH<sub>4</sub> mole fractions at the Earth’s surface from the four observational networks (NOAA/ESRL, AGAGE, CSIRO and UCI). "**

*Line 1739: And you show GATM for the previous version of global CH<sub>4</sub> budget (e.g. in Table 3) but not for this version. Why not?*

We thank the reviewer for this important remark. We did forget to report the values in Table 3. This has been corrected in the revised version of the manuscript.

*Line 1745: By definition, a growth rate can not be both positive and stable. Instead of stable, you mean consistent or persistent?*

This has been corrected to **“positive persistent growth rates since 2007”**

*Line 1751 to 1755: Here a reader finds the recent growth values, nicely documented with appropriate uncertainties. Why do these values not also appear in Table 3?*

Indeed, this was a missing element in Table 3. The values have been integrated to Table 3.

*Lines 1756 and following, Satellite data. Important to show command of this data for for budget purposes but way way too much information here. Why write four or five sentences of description about a source that you - for valid reasons - can not use? Is this a budget discussion or narrative about the history of SWIR sensors? You define SWIR at least twice in this section. Compilation - good - but not followed by tight budget-focussed editing - bad.*

Indeed, such information is not relevant for the budget. This part has been removed.

*Line 1778: “retrieval approaches, Proxy and Full Physics. The proxy method retrieves” ????. A former sub-heading now buried in this text? We do not need all this information?*

This part has been modified to remove extra details. The text is now: **“Different retrievals of methane based on TANSO-FTS/GOSAT products are available: NIES (Yoshida et al., 2013), SRON (Schepers et al., 2012) and University of Leicester (Parker et al., 2011). The three retrievals are used by the top-down systems (Table S6).”**

*Line 1792: “only inversions using GOSAT retrievals are used.” After nearly 40 lines of text a reader finally finds this short conclusion. Please can someone apply a sharp red pencil to this manuscript - focus on the CH4 budget!*

The details on instruments other than GOSAT have been removed, and the paragraph now focuses on GOSAT only. The authors have kept the last sentence (though reduced) because on the previous budget SCIAMACHY was used, but not anymore.

*Line 1799: “ensemble of inversions gathering various chemistry transport” Why? To reduce uncertainties or provide independent validation for the CH4 budget terms (e.g. use top-down to constrain bottom-up) or to ensure that a long list of inversion modelers get their work listed and recognized via this global CH4 budget paper. I apologize if this reviewer’s questions seem uniformed, irrelevant or even rude. But pity the poor readers/users. They want to use this product to understand CH4 budgets! Instead, they confront pages of tangential descriptions replete with (obscured by) more and more acronyms. Seriously, am I reading the CH4 budget or a narrative recount of every CH4 activity that has occurred?*

The authors understand the reviewer’s feeling after such a long review. We might have missed an explanation of why two budgets (bottom-up and then top-down).

From the bottom-up budget, estimates of emissions from the different sources are built independently, without confrontation to other studies, and especially without constraints from the atmosphere. Doing so, the reader understands that summing-up all this estimate brings to too high total global emissions.

Using the top-down estimates is a way to introduce atmospheric constraint. However, inversions have caveats as well (good for the total fluxes, less good for the sectoral partition; good for the total, less good for the regional sources). Confronting bottom-up with top-down has help to improve both approaches, and shows ways for next improvements. The GCP methane budget is also a platform of communication through the many different communities working on methane; communication, which was clearly missing few years ago, is the key to solve the main issues of the methane budget.

We acknowledge that Sauniois et al. (2016) reports many activities with probably too much details, lying between a budget and a review. From now on, and to address most of the referees’ comments, the “review” part can be set aside and GCP will focus more on the budget, while reducing the review components (and also the co-author list).

*Lines 1801, 1802: “assume that this model range is sufficient to cover the range of transport model errors in the estimate of methane fluxes”. You ‘assume’? This assembly of world experts on CH4 uses an ensemble approach because they can’t (or won’t) distinguish useful from not useful inversions? Again, does this approach help quality of the budget? If so, quantify that. Does it help users? If so, tell us. Show us! Did the inversions prove useful in the previous CH4 budget? If so, how? Although these authors claim (lines 1807, 1808) intention to do flux assessment rather than a MIP, the section in its present description looks, smells and sounds like a MIP to this reader.*

First, this is not an inter-comparison study as the model set-ups differ and generally do not even use the same prior fluxes. This has been emphasized with the following sentence:” **This approach corresponds to a flux assessment, but not to a model inter-comparison as the protocol did not enforce the use of harmonized priors and inversion set-up, as well as the set of input atmospheric data.**”

Well, of course, some inverse systems may perform better than other. A model evaluation needs to be done to assess this (an on-going work beyond the scope of this publication). In the future, such evaluation criteria should be defined and evaluation would need to be done before the final analysis and discussed in the next update of the budget.

Waiting for this, including all the models is a conservative approach that allows to cover different unresolved uncertainties: model transport, set-up issues including prior set-up.

The mentioned sentence has been replaced by: **“Including these different systems is a conservative approach that allows to cover different potential uncertainties of the inversion, among them: model transport, set-up issues, and prior dependency.”**

*Lines 1844 to 1846: Here, deep in the manuscript where few readers will notice, we finally read a justification of costs and benefits of time span and time lag of the current product. Good discussion! But we should have read it in the introduction? It sets out temporal goals that guide the entire product!*

The reviewer is right. It has been moved accordingly, to the methodology section 2.2 (Period of the budget and data availability).

*Lines 1850 and following: This is a good useful orderly section! But: a) it repeats a lot of material from earlier; and b) a reader needed to work through - or, more likely, skip through - an unusually large amount of text to get here. Authors could argue that this section depends on all the prior detail. This reader/reviewer asks authors to consider how to make the user's 'life' easier rather than putting priority on including all details. Details can and should go in individual research papers. The budget should not replay, only summarize?*

The aim of the methane budget paper is to produce an update of the different sources and sinks of methane. Despite the feeling of the reviewer, Section 3 is a summary of estimates of methane sources and sinks from bottom-up approaches. The organization of the paper is presented in the introduction, the reader has the possibility to skip Sections 3 and 4 to go directly to the budget, depending of his interest.

*Line 1882: Introductory sentence implies treatment by the five source categories but discussion/summary that follows treats natural vs anthropogenic (with wetlands called out separately) but does not follow the five-category organization that you used earlier, for example. Revise this opening sentence to better convey what will follow?*

The discussions on each anthropogenic sector are quite short here. So we did not follow the five categories organization. The first sentence has been modified accordingly as follows: **“The global methane emissions from natural and anthropogenic sources...”**.

*Line 1914; “study derive significant changes in wetland emissions” derive? I think you mean ‘identify’ or ‘reveal’ or ‘point to’?*

Indeed. This has been changed to **“point to”**.

*Line 1919: We just read (lines 1875) that uncertainty ranges for, e.g. ocean, termites, geological sources have reduced (improved) in this version, but here the authors point to those same specific sources as plausible reasons for the top-down bottom-up discontinuity? Uncertainty goes down but discrepancy goes up? Mathematically I think that works but this discussion implies a substantial weakness in our understanding. This reader particularly wonders about permafrost in this regard (see opening comments above), not so much about the permafrost numbers but more about whether assigning permafrost emissions as natural emissions doesn't presently or won't in the future exacerbate this discrepancy?*

This sentence has been deleted and replaced by **“This discrepancy comes from estimates in “other natural” emissions (non-wetland freshwaters, geological sources, termites, oceans, and permafrost).”** Later in the text, we now include the permafrost, and state: **“Better constraining the estimation and partition of methane emissions between wetlands and non-wetland freshwater systems, including emissions from thawing permafrost, may be the key to reconcile top-down and bottom-up budget on natural sources.”**

*Line 1930: “Improved area estimates ...” By this point reader has seen this recommendation at least twice. It really rather belongs in a summary or future work section, not here? This entire paragraph repeats earlier text and belongs instead in a summary section?*

Indeed. This has been partly moved to the Section 6, which has been entirely re-organized...

*Line 1948: “which is about 30% (23%) of global methane” What does the 23% indicate here? About 30% but actually 23%? 23% of global but 30% of anthropogenic? Some plus minus 23% uncertainty? I find the*

*answer to my question in line 1949, 30% of top-down but only 23% of bottom-up. Readers should encounter definition before data, not the other way around?*

This has been rephrased as follows: **“which is about 30% of the top-down global methane emissions, and 23% of the bottom-up total global estimate.”**

*Line 1958: “partition of methane emissions between wetlands and freshwater systems should still receive a high priority” this includes the thawing permafrost freshwater confusion?*

The reviewer is right that the permafrost issue is not fully considered in the discussion, and that, potentially, methane emissions from permafrost are included either in the wetland estimate (if satellite wetland areas cover peatland or thermokarst in permafrost regions) or in non-wetland freshwater emissions, or both. The sentence has been reformulated as follows: **“the estimation and proper partition of methane emissions between wetlands and non-wetland freshwater systems, and emissions from permafrost ecosystems, should still receive a high priority”**

*Lines 1982, 1983: Valid caution but a reader should have encountered it earlier?*

The authors think that this statement needs to be kept here, following the results. However, we have added in Sect. 4.2 the following sentences: **“In poorly observed regions, top-down surface inversions may rely on the prior estimates. Further, inversions bring little or no additional information to constrain (often) spatially overlapping emissions (e.g. in India, China) even in well observed regions. We recall that many top-down systems solve for total fluxes at the surface or for some categories that may differ from the GCP categories.”**

*Lines 1997 to 1999: Very confusing here, again the source/sink flux direction problem. The text says ‘model inputs somewhat higher’ but what you mean is some models show a higher value for total sink because they include a larger land sink term? For a known atmospheric CH<sub>4</sub> concentration, that larger sink term must lead to a correspondingly larger source term! Here, we read about a larger central top-down estimate. Authors intend this phrase to refer to the larger sink term, or to a larger total CH<sub>4</sub> emissions term? The authors, no doubt, know various sink, source and net terms, but here they fail to express themselves in clear language, leaving readers therefore confused. This statement occurs within the section on CH<sub>4</sub> sinks, so one wonders ...*

Well, the paragraph sits in the “sink section”. The authors agree that the meaning of the sentence was far from clear. It has been rewritten as: **“These sink estimates used as prior in the inversions are generally higher than the mean estimate of the soil sink calculated by bottom-up models (30 Tg CH<sub>4</sub> yr<sup>-1</sup>, Sec. 3.3.4).”**

*Line 2006: “chemical lifetime and to narrow it down in” Authors have addressed the issue of lifetime many times and in great detail prior to this statement. A reader knows at this point a lifetime of 9 years, plus or minus perhaps 0.5 years. How would refinement of this lifetime calculation improve the overall budget calculation, much less mitigation planning? E.g. does a lifetime of 8 years or 10 years really make a difference at this point? This reviewer suspects not. This section represents an(other) example of everyone’s scientific priority gaining equal weight in the compiled budget. This (small, to this reviewer) remaining uncertainty has larger overall priority than fixing the distribution and extent uncertainties of freshwater (also mentioned numerous times)? Organizationally, this entire section (lines 2005 to 2012) belongs in a summary / future work section.*

This part has been deleted from the text, and most of the ideas have been inserted, more clearly, in Section 6.

*Line 2024: A reader has now seen this statement at least four times?*

This sentence has been removed. The following sentence has also been removed to shorten the text.

*Line 2033: Readers already know, at least twice, why SCIAMACHY data not used.*

This has been removed.

*Lines 2024 to 2053: Good knowledgeable expert discussion of impact of satellite vs in situ data on inversions and emissions. But, of what relevance, in specific plus/minus Tg CH<sub>4</sub> terms, to the global CH<sub>4</sub> budget? A reader sees “clearly show” but at the same time “not systematically consistent”. I almost used a profane shorthand here: what do the authors want readers to learn, and how does it matter to the CH<sub>4</sub> budget? No clue in this section. More “further investigation” ....*

The text means to say: At the global scale, satellite and surface-based inversions give approximately the same global emission (consistent growth rate between surface and satellite observations). However, the regional distributions differ depending on the nature of the observations used (satellite or surface). Also the regional patterns of these differences are not consistent through the different inverse systems. Indeed, some systems suggest higher emissions in the tropics when using GOSAT instead of surface observations, while other suggest the opposite.

The “further investigation” part, has been removed from this section.

The text has been modified as follows: **“As expected, the regional distributions differ depending on the nature of the observations used (satellite or surface). The largest differences (satellite-based minus surface-based inversions) are observed over the tropical region, ranging between -13 and +26 Tg CH<sub>4</sub> yr<sup>-1</sup> below 30°N, and over the northern mid-latitudes (between -20 and +15 Tg CH<sub>4</sub> yr<sup>-1</sup>). Satellite data provide stronger constraints on fluxes in tropical regions than surface data, due to a much larger observational coverage. It is therefore not surprising that differences between these two types of observations are found in the tropical band, and consequently in the northern mid-latitudes to balance total emissions, thus affecting north-south gradient of emissions. However, the regional patterns of these differences are not consistent through the different inverse systems. Some systems suggest higher emissions in the tropics when using GOSAT instead of surface observations, while other suggest the opposite.**

**This difference may depend on whether or not a bias correction is applied to the satellite data based on surface observations, and the transport.”**

*Line 2082: Section 6, developments, missing elements, remaining uncertainties. Important section. Readers will not disagree with topics raised here. But the section reads more as a recitation of the previous CH<sub>4</sub> budget issues while omitting issues raised in this version. Ebullition, for example, mentioned repeatedly in the text, does not emerge here. The entire tension between spatial uncertainties and chemical uncertainties, a theme of this paper, does not emerge here, at least not in those recognizable terms. Pages of text on the need to improve distribution or lifetime of OH, but only one faint mention here, that basically repeats what we already read? We could get a list: previous but now resolved, previous but not yet resolved, new. Here a reader finds no sense of priority, just a large wish list not much changed from the prior budget. Use the GWP of CH<sub>4</sub>, find the largest or most tractable uncertainties, convert those to climate urgency: where should we put our efforts? If we know new satellites have recently or will soon come on line, what do we need for validation? Where will that data have most impact in this budget? More, more, more. More systematic. More integrated. Just another wish list. After all the work to compile this information, the world-wide experts then throw up their hands - they don't know what they need next? They need everything? Not helpful.*

*After working to assimilate all the details provided to this point, this reader basically glazed over this section. It seemed only vaguely related to what we just learned, a white -paper level of what we should do to better understand CH<sub>4</sub> but not well connected to all that we just read about efforts and information need to assemble this budget.*

Section 6 has been fully re-written in order to highlight the priorities and the time line of the needed measures to improve the budget and our understanding. The first item is on the wetland and freshwater issue, which emphasizes this point as number 1 priority. The authors feel that the newly written Section 6 resemble more a road-map than a wish-list from a white paper.

*Line 2089: “Knox and al., 2019” ??*

This has been corrected.

*Lines 2209 to 2219: a short clear paragraph of budget outcomes - thank you! Notable, unfortunately, because it differs so much in style, brevity and clarity from most of the preceding text.*

This is a comment.

*Lines 2226 to 2231: weaknesses and cautions about extracting regional information from satellites. Important, clear, useful. But link this to specific recommendations in Section 6, where now we just find a wish list for more of everything?*

Section 6 now better presents the recommendations for this issue; the authors refer to Sect. 6.

*Line 2237: a “clear priority”! Finally! First one? Does permafrost fit into this inland freshwater priority?*

*Line 2242: “also place importance in” Should a reader conclude here that authors consider improvements in OH distributions as second in importance to the freshwater extent problem?*

*Line 2246 to 2253: Short clear paragraph, contains a bit of the unhelpful ‘need everything’ philosophy, but if we have these topics listed here we could get rid of much of Section 6?*

*Lines 2254 to 2257: Good statement, good motivation, good summary of forward plans, but repeats almost verbatim what readers saw earlier (lines 1844 to 1846)? Some repeat expected in a good summary but not clear here why we need this detail twice?*

Response to the 4 above comments:

Indeed, the conclusion included many repeats from Section 6. This part has been highly reduced to avoid this.

*Line 2268: “a negative contribution (from biomass burning” You really do not want punctuation errors in such an important paragraph?*

This has been corrected.

*Line 2299 and following, Acknowledgements. Interesting section. I think global carbon budget does something like this but through a table instead? Nothing wrong with this approach but text includes several (perhaps many) punctuation and tense errors. Someone of the author team should read this carefully?*

For the CO<sub>2</sub>, budget the funding acknowledgement have been gathered in an appendix Table. We have done the same and put the acknowledgement to funding and support in Table A1.

*Page 126, 127, Table 3. Cells need reformatting, presumably will happen during typesetting. Authors will need to check text wraps. Repeat the question raised above about why no GATM rate information for current version(s)?*

GATM has been added. Table 3 formatting will be check during proofreading.

#### **References:**

Nakazawa, T., Machida, T., Tanaka, M., Fujii, Y., Aoki, S., and Watanabe, O.: Differences of the atmospheric CH<sub>4</sub> concentration between the Arctic and Antarctic regions in pre-industrial/pre-agricultural era, *Geophys. Res. Lett.*, 20, 10, doi:10.1029/93GL00776, 1993